

lished of British Labyrinthodonts. The authors, however, do not recognise the articular surfaces on the exoccipitals of *Loxomma* as the two condyles; and they speak of a concave articular surface as taking the place of a condyle or condyles on the basioccipital bone. The condyles in all Amphibia are produced by the exoccipital bones, and such a character is not a special evidence of the affinity of *Loxomma* with fishes. The number also contains several interesting papers on local natural history and antiquities, and the address of the president, the Rev. G. Rowe Hall.

LETTERS TO THE EDITOR

[The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.]

The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to ensure the appearance even of communications containing interesting and novel facts.]

The "Inflexible"

MY attention has been called to an article on the *Inflexible*, in *NATURE* (vol. xvi., p. 201), and I shall be much obliged by your inserting a few remarks, which I shall make as short as possible. On the general subject of the article I do not propose, nor would it be proper for me, to say a word. I am concerned only with the concluding remarks of the writer on a letter of mine to the *Times*. Nothing has appeared to me more astonishing than the use, or rather abuse, which is occasionally made of the report of Lord Dufferin's committee on ships' designs. If their authority can be claimed for any statement, I see on all sides a readiness to claim it. Should anything they have said militate against a favourite view, their authority is depreciated, and a comparison is sometimes invidiously drawn between the supposed opinion of the majority and that of an intelligent minority. Now if I be included with the unintelligent majority, I am quite content to find myself in such good company; but if, on the contrary, I am included in the minority, I utterly and absolutely refuse a compliment at the expense of my distinguished colleagues, with whom I shall always esteem it no small honour to have served. In fact I am not aware of any scientific point on which that committee was not unanimous. The writer of the article in question in common with many others, seems to have entirely mistaken the position of that committee. He seems to think their duty was to make their report a treatise on Naval Architecture. The absurdity of such a notion is apparent on the face of it. In fact they were required to give an opinion on certain designs of ships submitted to them as to their being in accordance with the latest developments of the theory of naval architecture. It was no part of their duty to descant on the principles which were successfully applied in such designs; but undoubtedly if they observed that in any direction caution was necessary, they were bound to remark it. In none of the designs was there any indication of a tendency to curtail initial and maximum stability of their due proportions; had there been they would certainly not have failed to call attention to it. But while they found the design of the *Devastation* in all respects sound, they yet thought it desirable that the range of stability should in future designs be somewhat enlarged. In recommending such enlargement they, by no means committed themselves to any such absurd dictum as the writer imagines—that range of stability is all that is requisite for the safety of a ship. But as I have already said, to have laid down all the other requisites of a good ship would have been to write a treatise.

Again, whatever credit according to some, or discredit according to others, is due to the design of a ship like the *Inflexible* with an armour-plated central citadel with unarmoured bow and

stern, that credit or discredit cannot be justly imputed to the committee. Mr. Reed, in his evidence, had brought a design with some of the leading features of such a ship before them, and it occupied a considerable share of their attention. Now what do they say on this subject?—"It is not by any means certain that some method may not be devised of securing the requisite reserve buoyancy by other means than armour plating." And after giving a sketch of what such a ship would be, they conclude thus:—"In the absence of any practical experience of the effect of large shells or of torpedoes upon such a structure as we have in view, it is impossible to say with confidence that the object aimed at would be thus attained, but, if it were, consequences of so much importance and value would follow that we think it right to indicate this line of inquiry as worthy of experimental investigation."

How far such a bare suggestion of experimental inquiry is from the recommendation of such a structure for adoption must be evident to your readers without further comment.

United University Club, Pall Mall, JOSEPH WOOLLEY
July 20

[The above letter from Dr. Woolley is what might have been expected from a man of his eminence in the science of naval architecture, writing under the restraint of his nomination by the Government to a membership of the Committee which is to report upon the stability of the *Inflexible*. It is no doubt to the concluding words of our first article on this subject (*NATURE*, vol. xvi. p. 203) that Dr. Woolley's letter refers, and we at once admit that there is very great force in the argument which he now employs. The particular point in question is a very simple one. In his letter published in the *Times* of July 19, Mr. Barnaby wrote:—"According to our estimate the ship, when fully armed, stored for fighting, and manned, will have, independently of the unarmoured ends—i.e., supposing them not to exist—a range of stability of 48 deg. The Committee on Designs considered that 40 deg. was sufficient range for a sea-going unarmoured ship." On the following day a letter appeared from Mr. Reed commenting on the impropriety of assuming the non-existence of the ends, pointing out that it was 50 deg. and not 40 deg. that the Committee spoke of as the minimum angle of vanishing stability, and adding that when the Committee put forward "range of stability" as "the one measure of safety" to be considered, "they stated the most dangerous doctrine which probably has ever been propounded in connection with the science of naval architecture." Now, on reconsidering the whole question, we are inclined to think that these words were not, in point of fact, quite fair to the Committee, because there was probably no member of the Committee who would have asserted or admitted that "range" was the one and only measure of safety to be considered. Dr. Woolley, Mr. Froude, Sir W. Thomson, and probably some other members of the Committee, doubtless knew perfectly well that the length of GZ from point to point was not only as important as "range," but far more important in all cases of limited range; and it is now obvious, with the present letter of Dr. Woolley before us, that the absence of any reference to the fact is attributable to the limited extent of the Committee's inquiry. There is great force in the remark that it was no part of the duty of the Committee to compose a treatise on naval architecture. On the other hand we are bound to deny that our remarks were penned under a contrary impression. Our view is that the use to which Mr. Barnaby has put the Report of the Committee proves that the scientific men who composed it would have done well to have employed more guarded language, and to have recognised in some manner the insufficiency of range only as a measure of safety. When they are found speaking of a certain angle of vanishing stability as being "sufficient to ensure the safety" of ships, it must be admitted, even by Dr. Woolley and his colleagues, that some risk of misconstruction was incurred. That misconstruction, or perhaps we ought in this case to say misuse—or even "abuse," as Dr. Woolley expresses it—has occurred in the present case is manifest, because Mr. Barnaby seized hold of the Committee's dictum as to range, and ignored altogether the very serious question of the amount of the stability. What makes the matter more important than usual in the present case is that the curve of stability due to the citadel of the *Inflexible* only is, no doubt, a low and flat curve, GZ being everywhere so small that in order to bring the stability up to a safe amount its range

would have to be very greatly extended. It was, no doubt, improper of Mr. Barnaby to make the use he did of the Committee's words, and Mr. Reed took no pains to credit the Committee with anything beyond what was written; but Dr. Woolley is, we think, a little forgetful of the fact that what Mr. Reed wrote, and what we have since written, has had to be said in presence of the circumstance that in a matter of the gravest public importance a free use of the Committee's words has been made by a high authority for the purpose of claiming for the *Inflexible* public confidence in her stability on the ground of range only. It is satisfactory to learn, however, on the undoubted authority of Dr. Woolley that the Committee, whatever its language, not only intended to give no countenance to the doctrine that the *Inflexible* would be proved safe if only she were shown to possess sufficient range of stability, but individually and collectively would consider such a doctrine as altogether absurd.

On the second portion of Dr. Woolley's letter, we entirely concur with him. We have read over again both the evidence and the reports of the Committee of Designs, and we cannot find the smallest justification for the assumption that, right or wrong, the Committee on Designs is responsible for this ship's design. The case to the contrary is absolutely clear and unquestionable. Mr. Reed placed before the Committee the outline designs of a ship of this description, but making it a *sine qua non*—let it in justice to him be said—that the ship should not depend “in the least degree” upon the ends, and that the stability of the citadel should be so ample as “to make it a matter of perfect indifference how much the ends might be knocked about by shot and shell.” He spoke of the ends as being filled with water, and thus converted into a sort of tanks, and it most naturally occurred to the committee to suggest whether cork or some metallic cellular material, might not with advantage be employed to take in some degree the place of water, a proposal which Mr. Reed thought well worth consideration and trial. Beyond this the Committee did not go in their report, as the quotations cited by Dr. Woolley clearly show; on the contrary, by recommending the course of experimental investigation which they advised they plainly showed that, in their opinion, sufficient grounds for depending upon cork, &c., for stability did not exist, and could not be shown to exist except by large and well-considered experiments. Mr. Barnaby roundly asserts that the Committee “did not agree with Mr. Reed's view as to the necessary dependence of the ship upon her armoured citadel for her floating power;” but the extracts from the Committee's Report which he adduces in support of the statement by no means bear it out. The Committee, for some reason or other, advert to Mr. Reed's plan without mentioning his name, but, while nowhere implying any dissent from his main principle, they plainly enough indicate that armour should be employed to sufficiently protect buoyancy and stability, unless “other means than armour-plating” could be found and proved effectual.

We shall defer to Dr. Woolley's very proper wish to restrict his remarks to the two points above considered, and shall in no way seek to connect them with the general question upon which he has been appointed a judge. We may be permitted to observe, however, that whatever the result may be, it is a satisfaction to us to find that the Committee consists of gentlemen who are in large part not merely masters of the science of the stability of floating structures, and raised high by their individual repute above the suspicion of partisanship, but who also, by serving on the Committee of Designs of 1871, acquired very special fitness for promptly considering the *Inflexible* case. They will know how to go directly to the questions at issue, and after ascertaining what stability the ship actually possesses without aid from cork, or canvas, or other devices, and what she possesses with such aid, they will be able to declare with scientific confidence and precision whether it is or is not sufficient, for they are themselves the authors of the very standards by which that issue must be decided. Nor will they forget that whatever demands for stability existed in 1871, still greater demands now exist when we have the First Lord of the Admiralty, in his place in Parliament, claiming for this very ship the ability to float and fight even after three successive blows from Whitehead torpedoes. If the result should be a disproof of our views of the subject, we know that that disproof will be based upon scientific grounds that will commend themselves to impartial minds. If the result should be to require that additional stability shall be provided in such ships, a great public good will have been accomplished. We need not say which result we anticipate.—ED. NATURE.]

The Manufacture of Leading Articles

THERE is a good old story told of a country editor who once met a pressing demand for copy in a singularly ingenious manner. At the moment of going to press, it was found to the consternation of the printer that a whole column was lacking. What was to be done? The whole staff was in confusion at the unexpected discovery; the editor alone preserved his wonted coolness. Sending for a copy of the *Times*, he clipped therefrom one of the leaders and ordered it at once to be set up in type, prefaced by the words “What does the *Times* mean by this?”

This story recurred to me with some force on reading on the front page of *Land and Water* last week, an article on Soldiers' Food in War; for the original, bearing my signature, appeared on the front page of NATURE (vol. xvi. p. 157). In this case, however, my other self seems to have had more time on his hands than the country editor, since the article in question has been paraphrased in parts, still with such care as not to destroy the identity.

H. BADEN PRITCHARD

July 24

The Fish-sheltering Medusa

PROBABLY the species of fish to which Mr. Lawless refers as seeking shelter under the swimming-disc of *Aurelia aurita* (NATURE, vol. xvi. p. 227) is *Merlangus carbonarius* (Cuv.), popularly called boat-fish. At least I have seen the fry of this species behaving as Mr. Lawless describes.

The observation stated in the following words appears to me one of great interest:—“Occasionally the Medusa turned in its pulsations, so as to bring the umbrella undermost, when the fish would shoot hastily out, but the Medusa had no sooner righted itself, than the fish returned.” Now, if this occasional turning on the part of the Medusa was not merely accidental, but, as Mr. Lawless implies, a reflex act performed with the view of escaping from the irritation occasioned by the fish, the fact would show that the marginal ganglia of *Aurelia aurita* are so far co-ordinated in their action as to enable the animal to steer itself in any required direction. For my own part, I have not as yet been able to satisfy myself that such ganglionic co-ordination occurs in any species of covered-eyed Medusa; so it would be well worth while if Mr. Lawless could repeat his observation a sufficient number of times to exclude the supposition of the somersaults being merely fortuitous.

I may take this opportunity of saying that the cut which illustrates the abstract of my lecture on p. 232 of the same issue of NATURE as contains Mr. Lawless's letter, is intended to represent the species of Medusa to which he refers, viz., *Aurelia aurita*. The cut is about $\frac{1}{2}$ natural size.

GEORGE J. ROMANES

Phyllotaxis

I HAVE noticed in the laurel and the Spanish chestnut species, in which the leaves have normally a distichous arrangement, that when a vigorous shoot takes a vertical direction—for example, after the stock has been cut down near the ground—the leafage of such a shoot is often quincuncial. The phenomenon suggests three possible interpretations. Is this to be regarded as a fixed adaptive habit, the spiral phyllotaxis being the fittest for the upright, the two-ranked for the more numerous lateral twigs? Or are the exceptional instances endeavours after greater economy of space in the packing of the buds? Or, finally, ought we to discern in the peculiarities of the more vigorous shoots a reversion towards some ancestral condition? W. E. HART

Drumawear, Greencastle, July 20

Printing and Calico-Printing

As all that I am ever personally concerned to know is the truth of a matter, I am glad to stand corrected by the writer of the article on Calico-Printing in the “Encyclopædia Britannica.” The claim I made, however, for the author of the “Natural History of Enthusiasm” was not my own invention; and it would be of interest, I think, to the many who must still, even in our day, revere his memory, to know more fully and accurately what it was that engrossed so many years of his valuable life, and what, if any, have been the practical results.

Bregner, Bournemouth, July 23

HENRY CECIL